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6. AUTHOR(S)

Hans Hoermann (1)
Pamela Tsang (2)
Michael Vidulich (3)
Amy Alexander (4)

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German Aerospace Center, Linder Hohe, 51145 Koln, Germany (1)
Psychology Dept., Wright State University, 3640 Colonel Glenn Hwy Dayton, OH 45435 (2)
711 HPW/RHCP, 2510 Fifth Street Wright-Patterson AFB, OH 45433(3)
MIT Lincoln Laboratory, 244 Wood Street, Lexington MA 02421(4)

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14. ABSTRACT
This chapter is based on the Researcher Plenary Panel held at the International Symposium on Aviation Psychology (ISAP) in May of 2015. This Panel followed the Practitioner Plenary Panel held on the preceding day of the symposium. The overarching goal of the two sessions was to foster a dialogue between operational personnel and researchers towards a safer and more efficient sky. The charge to the practitioner panelists was to inform the aviation community of their operational challenges. Their thoughts and discussions are captured in Chapter 2 of this volume. The charge to the researcher panelists was to explore best approaches that would bridge the gaps between basic research and current practical applications. The value of use-inspired basic research was discussed to a great extent by Stokes (1997). That use-inspired research would be a good path towards accelerating the process of putting basic knowledge to practical use will be revisited in this chapter.

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Researcher Role in Aviation Operations

Hans Höermann

German Aerospace Center

Pamela S. Tsang

Wright State University

Michael A. Vidulich

Air Force Research Laboratory

Amy L. Alexander

MIT Lincoln Laboratory

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The chapter begins with briefly presenting the missions of an example research and development organization to offer a glimpse of its research operations. Research approaches and practice toward a safer and more efficient sky will then be discussed. Since collaborations between researchers and practitioners are critical for the success of use-inspired research, means to facilitate their collaborations are explored. Finally, a call for action moving forward will be presented.

Missions of Research and Development Centers

To provide a glimpse of the natural habitat of researchers, the organization and missions of the German Aerospace Center (DLR) will be briefly described. The German Aerospace Center is a large R&D organization with many subunits. It has the mandate to respond to high level guidance and aviation psychology research is incorporated into an extensive multi-faceted portfolio that includes many scientific and technological domains.

German Aerospace Center (DRL)

Research institutions in aeronautics or astronautics, such as the German Aerospace Center (DLR) or the National Aeronautics and Space Administration (NASA) have to position themselves as to the extent to which they are committed to basic and applied research. This will be decided strategically by the stakeholders with regard to the availability of research facilities and workforce capabilities. Due to the culturally and politically diverse conditions in Europe and the European Union, harmonization, interoperability, and mutual alignment have always challenged the definition of any large-scale research agenda such as DLR.

The DLR has roots back to the "Aerodynamic Research Establishment" which was founded in 1907 in Goettingen. In the 1960s and 70s it merged with several other aeronautical research institutions and became in 1969 the German national research institution for aeronautics and astronautics with about 8,000 employees and its headquarters are in Cologne. The DLR sees its general mission in addressing societal questions on behalf of public customers by conducting research that enhances global mobility and safety while preserving environmental resources. It has committed itself to bridge the gap between basic research and innovative applications and to transfer knowledge and research results to the industry and the political sphere through mediation and consultation as well as through the provision of services. Specific enablers for these strategic objectives are 33 discipline oriented institutes and contractually regulated national and international partnerships with universities, industry, other research organizations and the public.

Air traffic has recorded substantial growth worldwide over the past decade, which, in all likelihood, will continue. Such growth cannot be sustained without consequences for the standards and requirements that the air transport infrastructure must meet. For example, questions arise regarding the impact on the environment and climate with ever-increasing urgency. Mobility, communication, climate change, demographic development, shortage of resources, safety and security are among the grand challenges of today. How can research in aeronautics help to enable sustained mobility in a demand-oriented, future-proof and environment-friendly manner?

Key features of DLR's research agenda lie in the holistic consideration of the air transport system as essential for achieving future objectives. International, and multidisciplinary collaboration is an indispensable condition to treat these complex questions with greater effectiveness. For example, a long-term cooperation agreement with NASA has recently been renewed. With respect to aviation this agreement currently includes collaborative efforts in aerodynamics, air traffic management (ATM), and climate research.

The basic structure of DLR's aeronautics program was established in 2007 to align with the European Vision 2020 (European Commission, Group of Personalities, 2001) and the corresponding strategic research agenda. DLR's main aims are: (a) to increase the efficiency of the air transportation system; (b) to increase the cost-effectiveness of development and operation; (c) to reduce aircraft noise and harmful emissions; (d) to increase the quality of air transportation for passengers; and (e) to increase safety in the face of growth and external danger.

In this research program, the applied industry relevance should also be associated with high scientific aspirations. To this end, the program promotes specific application-oriented projects. However, basic research also maintains an appropriate status. Among the currently promoted research topics are listed below:

- Extended capabilities to analyze and evaluate the overall air transportation system, which includes air traffic, airports and flight guidance, taking weather and environmental aspects into account. This aims at the performance optimization of the entire air traffic management (ATM) system including its environmental compatibility.
- Development of simulation procedures to support design, evaluation and certification programs in aeronautics. This aims, for example, at the expanded range of helicopters to all weather conditions.
- Further development of experimental techniques, equipment and systems for validating technologies and simulation tools in ground and flight tests. The research priorities include for example detailed investigations of fossil-based and alternative fuels, reliability, ignition and combustion stability in commercial jet-engines.
- Work for the human-machine interaction in the areas of the cockpit, cabin and air traffic control work areas, taking psychological and medical aspects into account. This includes human factors research on adaptive pilot and controller assistance, displays and sensors

for improved handling qualities of the aircraft, increased safety, reduced workload and enhanced situation awareness during the entire operation.

- Research work for a better understanding of the climate impact of air transportation and especially for suitable emission reduction measures in all areas of the air transportation system.

Transcending across all these fields, the research work is supported by a broad range of large scale facilities - such as wind tunnels, research aircraft, cockpit and tower simulators, combustion chambers and several test facilities for turbines, structures and materials – as well as the necessary infrastructure for demanding numerical simulations. These facilities provide excellent conditions to pursue research programs in line with international, primarily European, strategies, objectives and funding measures such as European Vision 2020 and Flightpath 2050 (European Commission, High Level Group on Aviation Research, 2011).

Research and Practice towards a Safer and More Efficient Sky

Generation versus Exploitation of Knowledge

For many people, including practitioners and students, the word *science* is often associated with hard study, complicated mathematical equations, voluminous textbooks and busy scientists wearing white lab coats while talking among themselves. While such impressions may be based on individual empirical experiences they reflect only a few aspects of science, which leans towards the acquisition of new knowledge by basic, laboratory research efforts. In the long run, science is driven by societal and market needs that have been identified and prioritized by policymakers, industries, and research organizations. In this context, science is undeniably applied, solution oriented and focused to achieve strategic objectives specified in local, national, or international research agendas. Since societal, market, and environmental conditions are constantly changing, the search for and discovery of new knowledge is an ongoing process to prepare mankind for future challenges. Therefore, as two sides of the same coin, basic and applied science cannot exist without each other. Indeed, successful attempts already have been undertaken in aviation and other domains to establish stronger connections between what is needed by the society and what can be provided by science in terms of established knowledge and solutions.

We will first take a closer look at the differences and communalities between the applied and basic approaches. Advantages and disadvantages can be found in both types of approaches along the following dimensions.

a) Explanation --- Solution. By identification of relations between different sets of factual information, basic science reduces the complexity within a real-world domain and generates theories and models that provide explanations for the outcomes of the human-machine system under investigation. Applied research strives for procedural knowledge, which can be utilized directly and provides solutions to operational needs.
b) Controlled Conditions --- Natural Conditions. Basic research is usually conducted within a laboratory environment with tight control of the experimental conditions that affect the observations. Since applied research is oriented towards applicable solutions, much of it takes place within the natural context in which the findings will be utilized.
c) Scientific Rigor --- Customized. The awareness and control of as many potential error sources as possible is one of the main quality criteria of basic science. Applied science has to adapt its methods and instruments in accordance to the contextual conditions within the field of application. The dynamics of these conditions follow the natural flow and cannot be fully predicted.

d) Internal validity --- External validity. Because the experimental conditions can be controlled more easily in laboratory environments, results from basic research often are accorded a higher degree of internal validity. This means more conclusive explanations can be formulated and findings are more likely to be replicable under equivalent conditions. However, since certain potentially influential factors in the natural conditions are sometimes intentionally disengaged in the laboratory in order to achieve better experimental control, the transferability of findings to the world outside of the laboratory is often limited. Experimentally demonstrated phenomena often cannot be observed in the natural environment.

e) Frequent Publications --- Infrequent Publications. Basic research is primarily conducted in universities and research organizations, where the holy mantra is "publish or perish. " In contrast, much of the results of applied research are not published. Although many scientific journals intend to publish more applied research findings in order to attract a wider audience from industry, the pragmatism of applied research is often criticized as not meeting general scientific standards. In addition, applied research results can affect commercial interests of an organization. Therefore, the interest to publish is sometimes overruled by the need of an organization to protect its propriety knowledge, practice, and technologies.

f) Reduced risk --- Elevated risk. The level of risk involved in the execution of basic and applied research differs significantly. With many factors under control and a lower complexity of possible interactions, the outcome of basic research is more readily foreseeable. Applied research often carries an elevated level of risk because of the higher probability of unexpected side-effects of an intervention or potential interactions with uncontrolled contextual influences. At the same time, results from basic research may carry the risk of not being generalizable beyond the highly controlled laboratory environment.

g) Proactive --- Reactive. Applied research is solution-oriented. This means that the researcher usually is tasked by a customer or a sponsoring body to provide solutions for an existing problem. The sponsor typically has full control of the research question. The time-horizon for a return of the investment is rather short because the application of research findings often can start without delay, dependent only on political decisions. Results of basic research are usually more abstract and general. Applicability of the findings is not always immediately apparent. General explanations, principles, models and even identified problems are the expected outcomes from basic research. Because of the generality, such findings could also be valuable for addressing new issues emerging in the future. Funding for basic research is in most cases provided from public sources such as scientific foundations. The sponsor has less or only indirect influence on how exactly the funding is spent.

It is however important to note that these distinctions between basic and applied research are neither dichotomous nor clear cut. In fact, Stokes (1997) argued that research at the intersection of the two approaches where insight and usefulness can be of equal value may be particularly fruitful. Applied and basic research each has its merits and contributes to the development of the other. We provide further evidence of the fact below and advocate leveraging the strengths of each.

Garnering the Fortes of Applied and Basic Research

In an attempt to objectively evaluate the relative merits of applied and basic research, Adams (1972) examined the results of two projects. One was Project Hindsight that was conducted by the United States Department of Defense (Isenson, 1967; Sherwin & Isenson, 1966). In this project, 20 weapon systems were examined, among them were defense systems such as the Minuteman ballistic missiles, the Mark 46 antisubmarine torpedo, and the Starlight Scope for passive night vision. The procedure was to work backward from an important innovation in the system and to ask what R&D events were responsible for it. These R&D events were traced back 20 years to 1945. Over 700 events were found, 91% of which could be classified as applied research. In 98% of these cases, the investigator was motivated by his awareness that a problem existed, not by pure scientific curiosity or the pursuing of knowledge. Also, 67% of the research events occurred before the specific system that it was applied to was begun, with a median time of nine years between occurrence of a research event and its use in a weapon system. In other words, impactful research is not short-term research that is done on the system itself, but is often relatively long-term work that occurs well ahead of immediate need. Further, the R&D events were found to have influenced not just a specific weapon system but were applied to a multitude of systems.

The second project was called TRACES (Technology in Retrospect and Critical Events in Science, Loellbach, 1968, 1969). The project examined long-term and basic research events as well as short-term applied ones without any time restriction. Critical R&D events associated with five socially important products: magnetic ferrites, the videotape recorder, the oral contraceptive pill, the electron microscope, and matrix isolation were identified. Over 300 R&D events were found and they were classified into three categories: basic research, applied research, and development and application. As was found in the Hindsight project, most of the applied R&D

events occurred 20 years prior to product innovation. But the basic research contributions came even more years before product innovation, peaking 20-30 years before the innovation and mainly preceding applied research. Moreover, basic research was far more influential than applied research. Basic research, applied research, and development and applications were found to be responsible for 70%, 20%, and 10% of the significant events respectively.

The two projects that Adams described convincingly demonstrated the value and impact of relatively long-term research and thereby the need for researchers to continue to build basic knowledge with rigor. But the time lag between discovery and application could also be significant. Stokes, Adams, and many others recognized the relationship between basic research and applications need not be serendipitous (see also Helton & Kemp, 2011). Allowing practical needs to inform the basic areas that need more intensive research would be expected to reduce the lag.

For example, Gopher and Kimchi (1989) identified three broad human factors topics that could benefit from better understanding of the underlying psychological principles: visual displays, mental workload, and training of complex skills. With regard to the topic of displays, recent technological developments have afforded practically infinite display format possibilities. Gopher and Kimchi proposed that one principle that should be used to guide display designs is the principle of representation. This is because the most effective display format would probably be the one that is most compatible with how information is represented by the human operator. Therefore answers to basic questions on how information is represented are likely to benefit many systems and not just a specific display of a specific system. With regard to the topic of workload, because increasing computerized automation has been permeating ever more human-machine systems; there is a growing need to be able to know how the system is performing and to be able to predict how the system might perform. Measures of mental workload have been introduced to augment or complement vanishing observable manual responses or complex performance that evades simple quantification. Gopher and Kimchi proposed that an understanding of the variables that underlie changes in mental workload would be tremendously useful. With regard to training, although automation has certainly increased system capability, it also introduces complexity. This is made plain from the abundance of testimonials from our practitioner colleagues in Chapter 2. Gopher and Kimchi proposed that in order to provide effective and expedient training, we need to have a strong basic understanding of how learning takes place. Instead of reinventing the wheel for each singular training occasion, research on basic principles of skill developments will need to continue.

Importantly, Gopher and Kimchi argued that sound principles should be applicable across a wide range of human-machine systems. For example, a sound training principle should be just as effective training a fighter pilot, a RPA pilot, or an air traffic controller. Further, sound principles should transcend technologies of the day. The existence of unique requirements in each training situation notwithstanding, validated training principles such as the importance of feedback and the incorporation of appropriately challenging elements should apply whether one is learning how to fly a plane with no engines, propellers, or jet engines. Although the years since 1989 have seen great advances in many technologies such as sensors (both environmental and physiological), displays (such as 3D and virtual displays), and especially computer hardware and software (which enable unprecedented levels of automation), the principles that Gopher and Kimchi (1989) referenced are no less relevant today.

Role of our professional organizations. Adams (1972) further suggested that professional organizations could play a more active role. For aviation psychology this would include such organizations as the Applied Experimental and Engineering Psychology Division of the American Psychological Association, the Human Factors and Ergonomics, Society (HFES), the Association for Aviation Psychology (AAP), and the European Association for Aviation Psychology (EAAP). All of these organizations could be more proactive about identifying the most profitable topics for basic research that would be likely to support the most effective transition to applied needs. Individual researchers would, of course, still plan their individual research projects, but Adams suggested that a collective vision of research priorities could help direct efforts to topics that have particular current relevance. And many such topics could be gleaned from the challenges laid out by our practitioner colleagues in Chapter 2. Their insights should serve as fertile ground for identifying knowledge gaps in our human factors and aviation psychology database.

But how can findings from highly controlled basic research be generalizable to the real-world? As Projects Hindsight and TRACES have shown, generalizations are not only possible, they are not an anomaly in science. Anderson, Lindsay, and Bushman (1999) considered the truism that laboratory research must have low external validity and field studies must have low validity and consequently little hope of bridging the two. They used meta-analytic techniques to examine the consistency of the effects of the same conceptual independent variables on the same conceptual dependent variables between laboratory and field settings across several domains in social psychology (e.g., weapons and aggression, gender and leadership style, age and job-training mastery). Thirty-eight pairs of laboratory and field effects were found based on a literature search of the major psychological journals. Across domains, the correlation of the effect size for laboratory and field studies was .73, a correlation considered to be large by convention (Cohen, 1988). The respectable correspondence between laboratory and field findings showed that the laboratory findings examined must have some external validity and the field findings must have at least some internal validity. Anderson et al. argued that laboratory research is by no means inherently internally valid and field studies not. Scientifically unsound studies can be conducted in the field as well as in the laboratory. That is, neither internal nor external validity is defined by where the study is conducted but by the method with which conclusions are drawn.

However, as Chapanis (1988) and many others have cautioned, generalizations are not guaranteed nor should they be assumed. But, there are a number of standard procedures of the scientific method such as having representative subjects, providing sufficient training, and using appropriate measures that would help improve the probability of generalizability. Chapanis also reminded us that one approach to support generalization over a wide range of situations is to purposely design heterogeneity into the studies. That is, the same relationship should be tested over different subjects (e.g., subjects with different levels of experience), tasks, response measures (e.g., decision time and decision quality), and environmental conditions (e.g., whether the human operator interacts with other humans or intelligent agents).

Beyond designing heterogeneity in individual studies, Gopher and Sanders (1984) advocated the back-to-back strategy for the overall research program. While the initial validation of a relationship between certain conceptual variables and dependent measures would mostly be done under tightly control conditions that typically use simple tasks, efforts need to be made to continue to test the relationship with higher degree of complexity that increasingly approximate that in the target environment to which generalization is to be made. Along the way, results with simple and more complex tasks are compared. This is not only to check the limits of the generality of the relationship being tested. This also affords an opportunity to reveal inaccuracies of the theoretical relationship and possibly ideas for a better representation, thereby contributing to the existing knowledge base.

Last, Wickens and McCarley (Chapter 5, this volume) discussed the potential drawbacks of the conventional overreliance on null-hypothesis significance testing for inferring practical significance. The issue has to do with the overemphasis on trying to avoid the error of incorrectly rejecting a null hypothesis or the error of detecting an effect that is not really present. The risk with this overemphasis is an increased probability of failure to recognizing an effect that is in fact present. While this is a concern for both basic and applied research, Wickens and McCarly argue that this may be especially problematic in the applied domains such as those involved with aviation safety. For example, failure to appreciate a true difference between two training methods because the difference did not reach statistical significance at the conventional level of $p < .05$, could lead to the failure of adopting a truly superior training method. While Wickens and McCarley are not advocating abandoning null-hypothesis significance testing altogether, researchers and practitioners are urged to also consider additional approaches, which they described in Chapter 5 (this volume).

Promoting Communications between Researchers and Practitioners

It is abundantly clear from our practitioner colleagues (Chapter 2) that there is much willingness, even desire, to work with researchers in a number of capacities. The need to work together is equally clear to researchers. But, as discussed in Chapter 2, there are considerable challenges to be overcome to enable better communications. Below are a few avenues for facilitating communications between practitioners and researchers.

Conferences and publications representing cross-section of practitioners and researchers. Scientific journal publications have long been the staple means for researchers to communicate with each other. Although basic and applied research tended to be published in separate journals historically, that is changing. There are now a growing number of publications with the professed aim of bridging basic and applied research. They include the *Journal of Experimental Psychology: Applied*, *Theoretical Issues in Ergonomics Sciences, Human Factors*, *Ergonomics*, *The International Journal of Aviation Psychology*, and *Aviation Psychology and Applied Human Factors*, just to name a few. Periodically, special issues where the entire issue is devoted to a contemporary topic receiving intense attention are put forth in these journals. For example, *The International Journal of Aviation Psychology* has published special issues on pilot selection (1996(2), 2014(1/2)), instructor training (2002(3)), aviation maintenance human factors (2008(1)), synthetic vision (2009(1/2)) and others. These special issues serve as a particularly excellent forum for researchers, airlines, manufacturers, regulators, and service providers to all examine and discuss the "real-world" requirements together with scientifically-proven solutions.

Although many of these journals are still primarily written by and for researchers, they all have the requirement that the authors make plain the relevance of the theoretical issues to applications. Some of them also encourage practitioners to submit papers not only to bring operational issues to the attention of researchers but again, to provide a common forum to engage both the researcher and practitioner communities.

Another category of publications are articles primarily written by researchers for practitioners to communicate new findings in a language accessible to them. One example is the *Ergonomics in Design*, published by the Human Factors and Ergonomics Society. Certainly, having more publication avenues that have the expressed aims of communicating with practitioners could incentivize researchers to work closely with practitioners in order to produce documents that are written in a language relatable to them.

A fourth category of publications are reports authored primarily by practitioners. The Aviation Safety Reporting System (ASRS, http://asrs.arc.nasa.gov/overview/summary.html) accepts voluntarily submitted aviation safety incident/situation reports from pilots, controllers, and others. The ASRS acts on these reports and identifies system deficiencies, and issues alerting messages to persons in a position to correct them. The authors are not aware of the existence of a similar “Hotline” system that would include not just reports of incidents but reports that identify human factors deficiencies or inefficiencies much earlier, prior to the occurrence of incidents. In the present highly networked world, such a reporting system might provide a more direct pathway for connecting practitioners and researchers early on in the problem solving process.

Even closer communication between practitioners, researchers and industry representatives is facilitated at conferences and workshops such as the annual meetings of the Human Factors and Ergonomics Society, the thematic lectures of the Royal Aeronautical Society, and the biennial International Symposium of Aviation Psychology and European Association on Aviation Conference.

Of note is that in 2014, the European Commission has launched the OPTICS project. OPTICS stands for “Observation Platform for Technology and Institutional Consolidation of Research in Safety.” It served as a platform for screening ongoing safety-related research and innovation activities in Europe. OPTICS organizes workshops and dissemination events once or twice a year. These workshops offer the opportunity to engage with policy and decision makers as well as leading aviation safety researchers to confirm promising research avenues and adjust the ongoing and future safety research agendas. Further activities are initiated to compile a living repository, which traces existing and ongoing research and innovation activities with relevance to aviation safety. This will help to strengthen the accessibility of already generated knowledge and solutions and to benchmark it against agreed strategic goals and upcoming industry needs.

Technological gatekeeper. Another approach for bringing the research to the practitioners and the practical challenges to the researchers is to develop the role of a technological gatekeeper. Adams (1972) suggested that this gatekeeper would read more of the professional engineering and scientific journals than the average practitioner or administrator and serve as a translator of basic science. Also, the gatekeeper would maintain a wide range of relationships with scientists and technologists outside of his organization. While there is likely to be someone in each organization assuming such a role already, Adams (1972) suggested such a role might be formalized and rewarded in order to maximize information transfer.

Organizational climate and institutional support. Institutional support for facilitating the activities described above is indispensable. Many of these activities like the support for publications, publication subscriptions, and conference and workshop participations, will undoubtedly incur costs but so do ineffectual designs, failed training, and unsafe operations. There are additional essential institutional supports that might entail a paradigm shift in the thinking at the operational sites as well as in the academic institutions. For example there needs to be mechanisms and reward structures in the workplace for practitioners to be able to participate in research collaboration much more fully than is currently typical (see Chapter 2). Similarly, academic researchers would need to be able to take certain risks in endeavoring less tightly controlled work that do not necessarily produce data that are acceptable only in theoretical journals. That is, not only would communications between researchers and practitioners need to be improved, government, industry, and academic administrators very much need to be in the loop as well. (See Figure 2.2 in Chapter 2).

Research funding scheme. An important lesson from Projects Hindsight and TRACES is that longer-term research in the end could have a much higher payoff. Consequently, an overemphasis on funding only work that seeks a quick but possibly only a one-time application is unlikely to be the winning strategy. This is especially important in aviation where the time needed to develop and deploy new aircraft or air traffic control systems can require many years. These lessons have made some inroads into many of the major funding agencies in the United States. At the same time, major federal funding agencies that have traditionally funded primarily basic research such as the National Science Foundation and the National Institutes of Health, have for some time now, required grant proposals to include explicit statements of the broader impact of the proposed research. The European strategic planning effort for the commercial travel system described below is another good example that seems to have heeded this lesson well.

At the Paris Air Show in June 2001 the Advisory Council for Aeronautics Research in Europe (ACARE) was instituted with over 40 organizations across Europe and representatives of the European Parliament. ACARE was tasked to find consensus on how aviation could better serve society's needs in the future. The result was the "European Aeronautics: A Vision for 2020" report (European Commission, Group of Personalities, 2001). Since we are presently approaching the year 2020, the vision was revised and published as "Flightpath 2050: Europe's vision for aviation" in March 2011 (European Commission, High Level Group on Aviation Research, 2011). Most of Europe's national and international funding schemes for research in aviation (e.g. Framework Programs, Horizon 2020, SESAR, CleanSky) are based on objectives - basic as well as applied - as defined in these documents.

An important element of ACARE's strategy is to establish a network for strategic research in aviation for the involved stakeholders (including industry, research establishments, academia, regulators etc.) and to facilitate stakeholder co-operation in Europe and internationally. For example, a number of specialized instruments in the Horizon 2020 and CleanSky programs are currently used to push innovation closer to the market and to stimulate the dialogue between academics, producers and end users. Thereby, a fresh contract between government and science is made, which will make the case for continued societal investment in realistic terms of the problem solving capacity of science.

Although much of the European 2020 and 2050 visions are directed at non-aviation psychology issues such as cleaner aircraft engines and recyclable aircraft, very ambitious goals for aircraft automation were identified. For example, Flightpath 2050 envisioned a future where, "Automation has changed the role of both the pilot and the air traffic controller. Their roles are now as strategic managers and hands-off supervisors, only intervening when necessary." (European Commission, High Level Group on Aviation Research, 2011, p. 9).
The means for achieving those goals, or indeed even proving their viability, will be a tremendous challenge for aviation psychology research and must include careful collaboration between researchers investigating the issues and practitioners that will ultimately need to use the resulting systems.

Forward-Looking, Future-Oriented Research

Innovative science does not only depend on available research facilities and financial resources. It is the breed of human curiosity, bright minds, team spirit, enthusiasm and creativity. These factors are often somewhat fluctuant and can hardly be scheduled by duty-rosters or stringent roadmaps. Ad-hoc solutions to current problems are rarely ingenious. Therefore, science policy makers and science managers have to consider that today's investment into organizational climate, individual promotion, and campaigning for young talents will serve our society's interests of tomorrow. Successful science management will have to find the right balance between controlling the expenditure of taxpayers' money and maintaining motivation and creativity-inspiring tolerances for research within the program-oriented funding schemes. This is not to say that we should neglect current market opportunities. It means that basic research even without a focus on immediate utilitarian thinking will serve tomorrow's needs if it explicitly addresses identified or likely trends into future problems. Following the philosophy of Antoine de Saint-Exupery: "Your task is not to foresee the future, but to enable it".

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